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THE
ACTION AND SOUNDS
OF
THE HEART;
A
PHYSIOLOGICAL ESSAY.

BY

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PREFACE.

The reader will find in the following pages considerable extracts from the writings of Billing, Bryan, and Brakyn, and at the end of the work a few Notes recording the opinions of trustworthy observers and writers on the subject under examination; the author's wish being to give his fellow-labourers all the merit that is their due; for he holds it to be expedient for the advancement of knowledge that each man of science should fairly acknowledge whatever help his mind has received from others.

London, 17, *Victoria Square, S. W.*
19*th December*, 1859.

PART I.

THE ACTION OF THE HEART.

It is singular that all previous inquirers into the subject of the Heart's movements, from Harvey downwards, have first removed the pericardium, and then made their observations. By this proceeding they have been confused, for before the removal of the pericardium the motion of the entire organ is natural, *i. e.* peristaltic; but after it loses its investing membrane it may truly be said to wabble, *i. e.* to have an uncertain or unequal motion. This explains Cruveilhier's description of the heart's movements, as detailed in the *Gazette Medicale* of August 7th, 1851. This eminent anatomist hastened to view these movements in a case of Ectopia of the Heart, and he has minutely recorded them; but he seems to have forgotten that he was beholding a *lusus naturæ*, not destined, because not perfected, for more than a few hours' fruitless struggles, for he tells us that the heart had *no pericardium;* the result of which was, that the heart under observation had no support, but by its own gravity made its fixed point where in the natural heart there is most movement, viz. at its connection with the aorta and pulmonary artery.

In Dr. Sibson's valuable essay, "On the Changes in the Situation of the Internal Organs," published in "The Transactions of the Provincial Medical and Surgical Association," vol xii. 1844, the heart's action is most minutely described; but, unfortunately, the pericardium was first removed,—a circumstance to be regretted, for from my own knowledge of the subject I am sure that the observations were most carefully made, and what was seen accurately recorded. Dr. Sibson's essay will well repay the reader's perusal.

I will first introduce to the reader's notice the substance of a paper to be found in "The Lancet" of February 8th, 1834, by Mr. Bryan, then of Stowmarket, now, I believe, residing at Cambridge. This paper is very remarkable, being purely theoretical, its author never having seen the heart act; but it may be safely taken, with a little modification and extension, as a key to comprehending a series of movements so rapid and apparently so confused as to have hitherto baffled the most eminent in anatomy, but yet so truly simple that any one acquainted with the structure of the organ may, by a careful perusal of Bryan's remarks, and comparing these with the results of my own repeated observations, at once see the order in which the several movements occur, and comprehend the necessity for the order so set forth. Bryan says—

As the heart consists of two sets of hollow muscles, alternately dilated by and contracting upon the blood, the investigation of its action necessarily requires some previous consideration of the physical state of hollow bodies distended by or contracting upon a fluid.

Let Fig. 1 represent the section of a hollow muscle in a state of relaxation, and having parietes of equal thickness throughout. By

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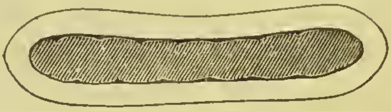

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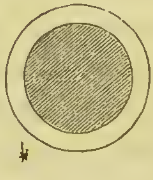

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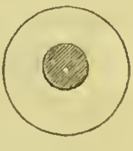

a well-known law of physics it would, when distended by fluid, assume a spherical form, Fig. 2; and in contracting and expelling a portion of its contents it would, of course, become a smaller sphere, as Fig. 3.

For the sake of elucidation, let us conceive a heart of the above simple construction. Let *a*, Fig. 4, represent the ventricles, *b* the auricles. The ventricles, *a*, in contracting would become a smaller sphere, as *c*, and, consequently, the parts in contact with their outer surface would be alternately dragged into, and pushed from, the space *i i* at each systole and diastole. Similar phenomena would attend the action of the auricles. The parts in contact with such a heart would be injured by its movements.

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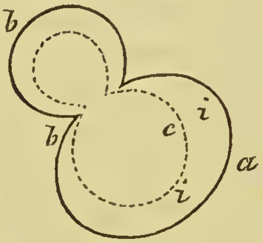

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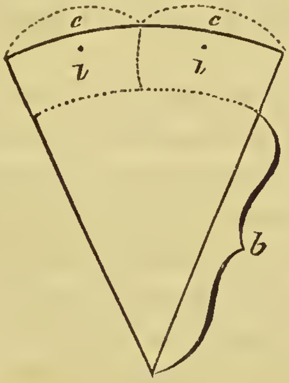

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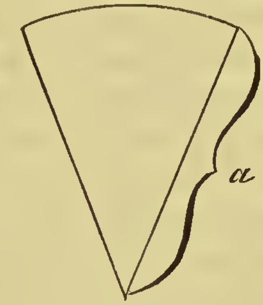

Let the strong lines, Fig. 5, represent the outline of a hollow, distended, muscular cone. In contracting it would become a smaller cone, as Fig. 6. It is evident the whole surface *a* of the lessened cone would be exactly adapted for the space *b* previously occupied by the lower portion of the cone in its distended state; and if auricles were situated at the base, as *c c*, they might, by distending in proportion as the ventricles contracted, fill the space *i i* from which the base of the cone had receded. From this it may be seen that conical ventricles, with auricles at their base, might contract powerfully without altering the form of the pericardium containing them, and, consequently, without agitation of the surrounding parts.

I would here wish the reader to consider what might determine the form of a hollow body during its distension by, or contraction upon, a fluid. Suppose that *a a*, Fig. 7, represents the section of a hollow muscle containing a fluid. When contracting upon its contents it would (as explained in connection with Figs. 1, 2, 3,) spring into the spherical form, as *b b;* but if it possessed bands stretched across its cavity, as *c c*, &c., it might, during distension and contraction, retain its original form. Different modification and arrangement of similar bands might determine in a hollow muscle any required form during distension, and any required alteration of that form during its contraction.

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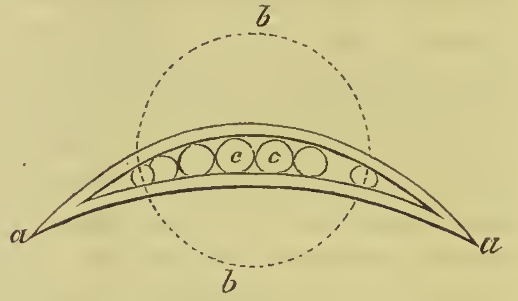
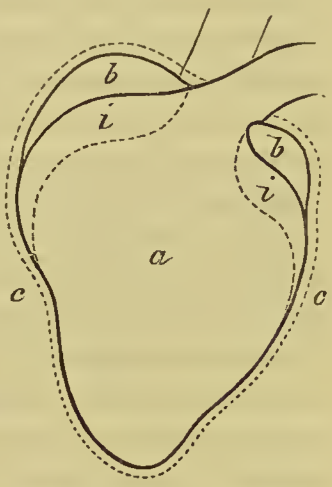


Then, because a conical heart can act without agitation, Nature has moulded the ventricles as nearly to the form of a cone as the surrounding viscera will allow without waste of space, and has made up for a slight deviation in this respect by the addition of internal muscular bands, which afford to the ventricles those properties which, without internal bands, the perfect cone alone possesses. The shape of the auricles being of secondary importance, they are so constructed as readily to adapt themselves to the varying form of the parts in contact with them; and, as the shape of their loose portions—the appendices—is extremely irregular, numerous internal bands become indispensable to their smooth and tranquil action. These muscular bands of the auricles and ventricles have much engaged the attention of physiologists, and various and unsatisfactory have been their opinions concerning them. They enable the heart to act smoothly.

Ventricles, without them, unless their figure were that of a perfect cone, would be a torment to the being to whom they belonged.

From what has been advanced I have been led to believe that the heart moves as exemplified in the following diagram.

Fig. 8.



Let Fig. 8 represent the heart *a*, ventricles *b b*, auricles contained in the pericardium *c*. Suppose the ventricles to have arrived at their extreme distension; the instant contraction commences the auricular valves are closed by the blood, but no important alteration in the size or form of the ventricles takes place until the pressure of the blood contained in their cavities becomes greater than that in the arteries, when the basilar portions sink towards the apex, as from the line below *b* (on the right) to the dotted line below *i*, occasioning partial or perhaps total obliteration of their cavities. While this depression of the base of the ventricles is taking place, the auricles *b b* swell into the space *i i* receded from, thus rendering it unnecessary for the pericardium, and, consequently, the parts in contact with it, to close in upon the contracted ventricles. During the diastole of the ventricles the reverse of this movement takes place, *i. e.* the bases of the ventricles rise into the space *i i*, from which the auricles withdraw.

It will naturally be questioned by what power the ventricles recede towards their apex, when, from their attachment to the aorta and pulmonary artery, one would be led to conclude that the apex and the other portions of the ventricles would be drawn towards the orifices of those vessels. Let us for a moment reflect upon what would be the movement of the ventricles if they were quite free. It is certain that, in ejecting their contents by the arterial orifices at their base, they would recede in the opposite direction—that of their apex. This motion would be serviceable with respect to the heart's minor function, that of receiving the blood, for by abstracting pressure from the auricles it would induce the flow of blood into them by the force commonly called suction. Could it for a moment be supposed that nature would use means to oppose this natural and useful movement of the ventricles, in order to establish a movement in the opposite direction, *i. e.* that of the arterial orifices, which must tend to trammel the dilatation of the auricles, causing them to swell in a direction upwards and backwards, while the ventricles, contracting towards the above points, must alternately drag towards and push from these points the lungs, &c. in contact with them. If the orifices of the aorta and pulmonary artery were the fixed points towards which the muscular fibres of the ventricles contracted, the heart at each beat would move in the manner of the simple heart represented by Fig. 4, produce much agitation in the chest, and occasion a *bruit de soufflet*, by causing the entrance of air into that portion of the lungs which is suddenly dragged into that space from which the substance of the ventricles must unquestionably recede in tending towards the above points. Such action as this is highly inconsistent with that delicacy and precision which nature displays in all her works. The obliquity with which the aorta and pulmonary artery arise from the ventricles would permit a slight descent of their orifices towards the apex, and the necessity for much movement of these parts might be, and I believe is, obviated by the posterior and lateral portions only of the base descending.

The last few lines prove that Mr. Bryan never saw the heart act, else he would not have made this reservation of the descent of the origin of the large vessels to his theory; for, as will be seen further on, they descend freely, this movement being favoured by the perfect elasticity of the arterial coats.

In prosecuting my own inquiries, asses and dogs have been the subjects of experiment, and the following method has been adopted: chloroform was first administered to produce insensibility, then the trachæa divided, and the nozzle of a bellows inserted into the lower portion, and artificial respiration maintained; the anterior portion of the thorax, viz., sternum, the cartilages of the ribs, and sometimes parts of the ribs themselves, according to the depth of the thorax, removed or turned upwards. In this process it is necessary, occasionally, to drop some chloroform into the valve of the bellows, otherwise the animal may become conscious, and disturb the operations.

Observation 1.—When the heart is thus exposed, the movements of the whole viscus, and alternately of its separate parts, can be studied through the more or less transparent pericardium. (Should the pericardium be loaded with fat, it can easily be dissected off without piercing the membrane.) The artificial respiration being properly and rhythmically kept up, the facility and smoothness of the heart's action strikes the observer, any impulse or beat of the heart not entering into the mind. The point of the finger coming in contact with the ventricles, immediately dispels the deception, and the *apparent* blow is distinctly felt. (1) A stethoscope, lightly applied, is raised against the ear, and the first and second sounds are distinctly heard.

Observation 2.—When the pericardium is removed, and its contained fluid consequently lost, the heart's action becomes immediately tumultuous; the contractions appear more forcible, and the sounds are much louder; and this is the character, more or less intense, of its action ever after. The pericardium, therefore, regulates the heart's movements.

Observation 3.—The first sound occurs during the contraction of the ventricles, and the second immediately follows it; that is, whilst the auricles are being filled with blood and no muscular action is going on.

It is necessary now to bear in mind, that the ascending portion of the arch of the aorta and the commencement of the pulmonary artery must be looked upon as heart when speaking of the movements of the latter; for they are equally contained within the pericardium.

Observation 4.—During the systole, or contraction of the ventricles, the base of the heart approaches the apex; the latter at the same time is pressed downwards, backwards, and from right to left, describing part of a circle; the ventricles, assuming a contracted globular form, descend, describing also part of a circle, but passing forwards and from right to left. There is also a forward movement of the left ventricle above the apex from left to right. These minor movements must be seen, they cannot well be described; but the facts of importance to be remembered are, that, when the ventricles contract, their bases descend towards the apex, and the latter is not tilted forward so as to give any blow to the thoracic walls, but is pressed downwards, its extremity being directed backwards and from right to left.

Observation 5.—The ventricles contracting, the auricles simultaneously receive blood and then occupy part of the space within the pericardium previously taken up by the ventricles in their relaxed distended state. (2)

Observation 6.—With each contraction of the ventricles and descent of their bases, the pericardial portions of the aorta and pulmonary artery become suddenly greatly elongated and distended, and as suddenly, by their elasticity, react upon their contents, becoming of uniform calibre, but visibly distending the sinuses of Valsalva. (3) The venæ cavæ are similarly distended during the contraction of the auricles.

Observation 7.—The finger and thumb spanning the diameter of the ventricles are perceptibly further separated during the contraction of the ventricles, and approximated during their relaxation.

Observation 8.—The same *apparent* blow is felt over every part of the ventricles during their contraction, but more forcibly over the centre. The impulse at the ribs is most probably given by the fibres just above the apex.

Observation 9.—The apex is the most fixed part of the heart. Great pains were taken to ascertain whether the eye was deceived or not. A piece of string was stretched tightly across the thorax, and a pin thrust through it into the substance of the heart. The string acting as the fixed point, the oscillations of the pin corresponded to the movements of the part into which it was inserted. Thus, when inserted about the centre of the sulcus separating the ventricles anteriorly, the head of the pin moved upwards during the contraction of the ventricles, and downwards during contraction of the auricles, that is, in a reverse direction to that of the heart and the pin's point inserted into it. In this situation the pin moved longitudinally along a line equal to |, whereas when inserted into the apex the distance did not exceed | . The same was tried over other parts of the heart, and always with the same result, viz. that the apex was more fixed than any other part. (4)

A careful consideration of these observations shows that the heart's action is extremely simple; that there exists a reciprocity or compensation between the auricles and ventricles, and that, although the chambers of the heart are continually receiving and discharging their contents, these actions are so regulated that the bulk of the contents of the pericardium is always the same. For instance, when the ventricles contract, the loss of the bulk of the fluid they have ejected is made up, or compensated for, by the bulk of the fluid pouring into the auricles, and by the increased bulk of the pericardial portions of the aorta and pulmonary artery. This is shown in figs. 9 and 10.

Fig. 9.

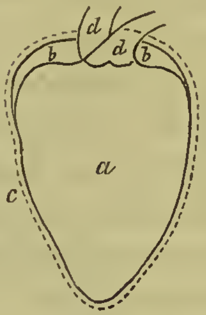


Fig. 10.

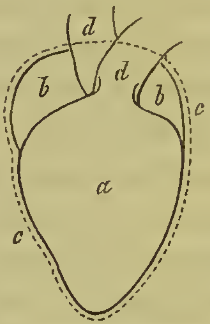


Fig. 9.—*a*, ventricles relaxed and full; *b*, *b*, auricles contracted and empty; *d*, *d*, aorta and pulmonary artery full, but not distended, the semilunar valves being shut down; *c*, pericardium.

Fig. 10.—*a*, ventricles contracted and empty; *b*, *b*, auricles relaxed and distending (they do not become fully distended until the aorta and pulmonary artery have, by their elasticity, reacted upon their contents, and have become of diminished calibre, the auricles still filling after the second sound is heard); *d*, *d*, the aorta and pulmonary artery greatly distended and lengthened, for observe, that the bases of the ventricles, from which these vessels spring, are lower than in fig. 9; *c*, *c*, the pericardium.

This uniform shape of the pericardium is however departed from at the instant of the ventricular contraction, the change of form causing the impulse which has erroneously been attributed to the tilting forward of the apex, whereas it may be caused by any portion of the ventricles in contact with the thorax or diaphragm. Were there no opposing column of blood pressing upon the upper surface of the semilunar valves, there would be no blow or impulse caused by the ventricular systole. How much this impulse is augmented and communicated to surrounding parts where diseased valves obstruct the circulation, is too well known for me to enlarge upon here.

We must, then, not forget, in our physical examination of patients, that any part of the ventricles being in contact with the chest-walls will give, during contraction, an impulse.

Let us now turn our attention to what is taking place in the interior of the heart simultaneously with the external conditions we have been noticing.

The cavities of the auricles and ventricles being at one time continuous with, and at another separated from each other, by what means and at what time are these changes effected? In short, what are the functions of the auriculo-ventricular valves, chordæ tendineæ, and musculi papillares? Also, What share does the blood take in these actions?

To determine these points I adopted the following method. A bullock's heart was obtained, and the auricles cut away nearly as low down as the auriculo-ventricular openings; the cavities of the ventricles were well washed out, and the coagula carefully removed. A vulcanised india-rubber tube of like diameter with the pulmonary artery was then attached by one extremity to the vessel, and by the other to a common forcing-pump; water was then thrown into the pulmonary artery, and the semilunar valves tightly shut down, gentle pressure being maintained, in imitation of what takes place during life. The right ventricle, being empty, was in the same state as when the auricle is about to inject it. On pouring water into the ventricle the flaps of the auriculo-ventricular valve rose upon the surface of the fluid, until (the ventricle becoming fully distended) the valve formed a perfect septum between it and the auricle. The left side of the heart was tested in the same manner, and with results perfectly the same, notwithstanding the greater thickness of the valve, the larger size of the musculi papillares, and the stronger chordæ tendineæ. Fig. 11 represents the right auriculo-ventricular, and Fig. 12 the left auriculo-ventricular valves closed without the least pressure on the sides of the ventricles.

The action of these valves has been previously shown to

Fig. 11.

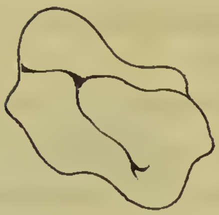
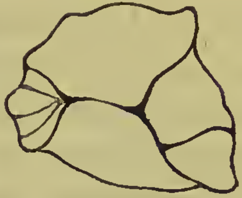


Fig. 12.



be passive. I find Valentin (5) quotes some experiments by Fick. He says, "These peculiar structures are best shown by pouring in water from the pulmonary artery and aorta, until it reaches to a point in these vessels which is somewhat higher than the region of the auriculo-ventricular valves." This, however, is a very different proceeding from that adopted by myself—the principle is different; nevertheless the true action of the valve is thus stated by Valentin further on: "The slightest pressure suffices to close the auriculo-ventricular valves—a very weak contraction of the walls of the ventricle—indeed, the mere force with which the blood is expelled from the auricle, and rebounds or is rejected by the elasticity of the ventricular walls, completely suffices for this purpose."

Dr. Hamernjk (6), recapitulating at some length the opinions of Dr. Baumgarten, says,—

> That the auriculo-ventricular valves are closed by the counter-pressure of the ventricular blood, suddenly developed by the contraction of the auricles; that the cavities of the auricles and ventricles during the heart's diastole are distended by the continuous current from the veins, while at this period the valves are to be found floating in the blood in the form of a funnel; that the object of the auricular systole is to induce such an amount of tension in the contents of the ventricles, and of course in the blood surrounding the funnel-shaped arrangement of the valves, as to cause their rapid closure; and in this way only can regurgitation be prevented.

Now, I grant that Dr. Hamernjk is perfectly correct when he states that "the auriculo-ventricular valves are closed by the counter-pressure of the ventricular blood suddenly developed by the contraction of the auricles;" but he is incorrect when he says that "the cavities of the auricles and ventricles are distended during the heart's diastole;" for, as I have shown by the diagrams Fig. 9 and Fig. 10, there cannot be full auricles and full ventricles at the same time within the pericardium. Nevertheless the truth came out of these experiments, as witness the remark of the Editor of "The Monthly Retrospect:"—"The experiment by which it is shown that the auriculo-ventricular valves are closed before and independently of the ventricular systole is very easy of performance."

Independently of the above authorities, I was led from experiment to treat of the closure of these valves as follows:—*The auricles contracting on the blood, the force of their contraction is transmitted by the blood in all directions, separating the flaps of the valves, distending the ventricles, and (the semilunar valves being shut down) pressing as much upwards and backwards, as downwards and onwards. The force, not being sufficient to raise the semilunar valves, is expended in distending the ventricles, and raising and closing the auriculo-ventricular valves.*

Let the fact of there being a perfect septum between auricle and ventricle, previous to the contraction of the latter, be taken in connection with the fact (as stated in Observation 4, and as shown in Fig. 10) of the descent of the bases of the ventricles during their systole, and we arrive at a certain conclusion respecting the functions of the musculi papillares. Seeing then that the valve forms a perfect septum as above, when the ventricle contracts as from *a*, Fig. 9, to *a*, Fig. 10, these muscles also contract in proportion as the base of the ventricle descends, and their tendons (chordæ tendineæ) being inserted into or connected with the under surface of the flaps of the valve, from apices to circumference, maintain the proper tension of the valve till the whole of the blood is expelled from the ventricle.

The auriculo-ventricular valve forms a perfect septum between the auricle and the ventricle as soon as the latter becomes distended, and is kept in this position during the whole ventricular systole by its own proper muscles (musculi papillares), which, by contracting simultaneously with the descent of the base of the ventricle, prevent the eversion of the valve into the auricle. (7).

The following Table will facilitate the comprehension of the heart's action, the time, and, as will be shown further on, the cause of its sounds (*see opposite page*):—

We now pass to the more difficult part of our inquiry, viz. The Cause of the Sounds of the Heart.

ANALYSIS OF ONE COMPLETE ACTION OF THE HEART.

Time occupied, say, one second.

Rhythm.	Time.	Order of Occurrences.	Muscular Action taking place.
Last Part of Pause.	$\frac{1}{8}$	Auricles contracting. Ventricles distended with blood by the force of the Auricles. Auriculo-ventricular valves closed.	Contraction of Auricles.
First Sound and Impulse.	$\frac{1}{2}$	Ventricles contracting on their contents; tension of the auriculo-ventricular valves and chordæ tendineæ; blood passing into and increasing the calibre of the Aorta and Pulmonary Artery. Auricles relaxed, permitting the flow of blood into them from the venæ cavæ and pulmonary veins.	Contraction of Ventricles.
Second Sound.	$\frac{1}{4}$	Ventricles relaxing; auriculo-ventricular valves and chordæ tendineæ no longer tense; small quantity of blood passing into Ventricles. Auricles filling with blood; calibre of the Aorta and Pulmonary Artery diminished by the elastic reaction of their coats on the contained blood, producing forcible closure and tension of the semilunar valves.	None.
First Part of Pause.	$\frac{1}{8}$	Ventricular fibres, auriculo-ventricular valves, and chordæ tendineæ, fully relaxed. Auricles distended with blood. *Here the requisite condition of both Auricles and Ventricles is arrived at for a new set of actions.*	None.

N.B. The above Table shows the contents of the Pericardium, from the Apex of the Heart to its connection with the Aorta and Pulmonary Artery, always to be the same.

PART II.

THE SOUNDS OF THE HEART.

THE second sound is almost unanimously believed to be due to the tension of the semilunar valves of the aorta and pulmonary artery. The cause of the first sound, however, is differently stated by physiologists, inasmuch as up to the present time at least thirty explanations have been offered of its production. Need I say how detrimental to the study, and discouraging to the student, of medicine have been this uncertainty and perplexity? The subject has by its occultness been peculiarly enticing to the speculative and fanciful; but it must not be left in such hands, it must be brought back and tested, both theoretically and experimentally.

Dr. Billing, in the preface to the second edition of his "First Principles of Medicine," thus speaks of the Sounds of the Heart:—

> In an essay read at the anniversary meeting of the Hunterian Society (February 9, 1832, and reported in "The Lancet," May 19, 1832), I first stated publicly that "the push or beat felt at the side is caused by the ventricular muscles, in their systole to expel the blood. The first sound is caused by the tension produced in the shutting of the auriculo-ventricular valves, and the second sound is caused by the tension produced in the shutting of the ventriculo-arterial valves, &c. . . . . . This is a simple, unsophisticated explanation of the causes of the beat and sounds of the heart; and you will find that the morbid signs are all explicable as alterations of these." Subsequently Rouanet brought

forward the same explanation in his thesis, which was noticed in the "Journal Hebdomadaire" (September, 1832), and copied into the "Medico-Chirurgical Review" (April, 1833), as well as an extract from my essay; in which I had advanced, as I thought, sufficient to enable any practised physiologist to confirm my positions by the suggestions of his own mind. Finding, however, from some observations, that this did not appear to be the case, I published the following additional remarks in "The Lancet" (November 30, 1833):—"The succession of phenomena of the heart's action is as follows:—First, the auricle contracts, then the ventricle; by the action of which latter the auriculo-ventricular valves are shut by the pressure of the blood against them (8). Upon the relaxation of the ventricle, the semilunar valves are shut by the backward pressure of the blood in the artery. The first sound takes place exactly synchronous with the impulsion and action of the ventricle; hence it might be supposed that the action of the muscle (as averred by some) produces the first sound. But the second sound takes place at a time when there is no action of the heart going forward; and this is peculiarly evident when there is an intermitting pulse, as there is then a marked pause after the second sound; so that, in fact, there is nothing but the semilunar valves in operation to produce sound at the instant." I have thus proved that the second sound can be produced by nothing but the valves; and I have therefore shown the tension of the valves to be a sufficient cause for the first sound; and as *nil frustra natura facit*, according to the laws of reasoning in physics, more causes than are true or sufficient are not to be assigned (Newt. Princip. lib. iii. Reg. Phil. 1), so I discard muscular action as the cause of the first sound.

Speaking of the conclusions arrived at by the Committee of the British Association, appointed to inquire into the cause of the first sound, he further says:—

Now, as to their experimentum crucis (to show that muscle produced the first sound), of putting the finger into the heart after the valves were destroyed, and their hearing a sound proceeding from the contraction of the heart, with air, carneæ columnæ, and bloody moisture in it, without the fingers,—doubtless there was *a* sound produced independent of the valves, but not *the* sound of the heart. I say my proof is legitimate—their assertion a sophism of *non causa pro causâ*. It is the tympanic sound of the membranous valves which, with the time of the beats, produces the rhythm; and we judge of the existence of certain states of disease by the degree and manner in which they are out of tune. Looking at the subject physiologically and pathologically, valvular sound is the one we have to depend upon; for granting, for argument's sake, even that any other exists as a normal accompaniment, it has no more to do with *the* sound than the drone of a bagpipe has to do with the tune.

Ignorant of Dr. Billing's priority, Mr. Bryan thus wrote ("Lancet," January, 1833):—

> I shall content myself with offering an explanation of the first sound, founded on a simple law of physics, examples of which are so frequently before us as to have rendered it familiar to every one.
>
> Any flexible solid, suddenly brought from a state of relaxation to a state of tension, vibrates, and its vibrations are sonorous or not, *i. e.* audible or not, according to its physical structure.
>
> At the commencement of the systole of the ventricles, their auricular valves are flapped into play, and at the instant of their closure the whole substance of the ventricles and the valves is suddenly brought to a state of tension, and then consequently vibrates. I leave it to the reader to determine according to the laws of physics whether the vibrations of the valve floating freely in fluid, or the muscular substance of the ventricles trammelled by the contact of surrounding solids, would most contribute to the formation of the first sound.

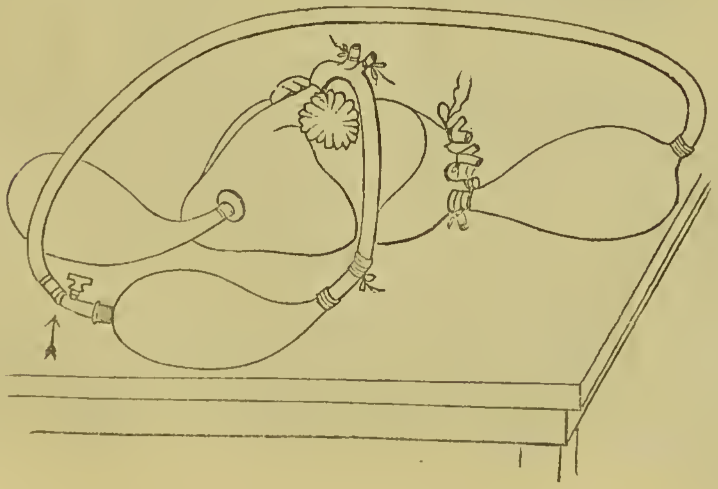
Notwithstanding the above ably-written theories, the valvular origin of the first sound had very few supporters.

We now come to the experimental part of the inquiry.

In order to prove that *tension* of the auriculo-ventricular valves is sufficient to produce the first sound of the heart, Mr. Brakyn of Dublin devised an ingenious experiment, the details of which I extract from "The Lancet," November, 24, 1849:—

The apparatus used consists of an ox's heart carefully dissected from the animal, so as to avoid injury to any of the appendages. To this I attached an apparatus consisting of a flanged tube, attached to the middle of the left ventricle, and piercing its wall, introduced through the auriculo-ventricular opening, to which was screwed externally another tube, with a flange also, so as to grasp the wall of the ventricle all round the tube, and render the junction air-tight; to the outer tube a bladder is tied. A free communication is thus established between the bladder externally and the cavity of the ventricle within. To the left auricle, a similar apparatus, but without flanges, was then attached by one trunk of the pulmonary veins, the rest being tied. Then, having tied all the offsets of the aorta, I tied a tube and bladder to its abdominal extremity; to the distal end of this a small stopcock was then tied, into which a brass pipe, fastened to the end of an india-rubber one, can be wedged; the other extremity of the caoutchouc pipe is finally attached to the distal extremity of the auricular bladder. There is thus completed an apparatus, permitting a mimic circulation through the left heart (it being sufficient for illustration), which may be conducted with perfect ease in the following mode:—

Fig. 13.



Let the system be inflated with air through the orifice of the elastic tube next the stop-cock (marked by an arrow in the diagram); when, having wedged back the stop-cock into the pipe and opened the cock, a rhythmical circulation may be carried on by alternating manual pressure applied to each of the three bladders in succession (without removing any of the three hands applied), thus representing the successive contractions of auricle, ventricle, and aorta, with the natural attempts at regurgitation, which close both sets of valves in succession. Hereby a complete imitation of the normal sounds may be produced on either a very magnified or diminished scale, according to the force used in propelling the air.

These sounds, being produced without any muscular contraction or rush of blood, &c. must evidently be valvular, which can be further demonstrated by removing part of the apparatus (the auricular) so as to show the mitral valves in action synchronously with the first sound, or by introducing a wire cage prevent them closing on regurgitation when no sound follows; above all, the first sound is as perfect as the second, the valvular origin of which is I believe undisputed. In fine, the illustration, though conducted with air, ought to be conclusive, inasmuch as a suddenly strained membrane, which gives a tympanic sound in air, will do the same in water also, as I have tried, but not so loudly.

I had the good fortune to witness and assist at this experiment, when a pupil of the Grosvenor Place School of Medicine. Mr. Brakyn, going abroad, left the apparatus with two of the pupils, Messrs. Waters and Sercombe, who, with myself, frequently repeated the experiment, which was quite conclusive as to the valvular origin of both sounds. There being still an objection in the minds of some to this experiment, and an almost total neglect of Billing's original theory, I determined, in 1851, to see what would be the effect of fluids upon the valves; whether the tension produced by their resistance to compressed fluid would produce sound—such sounds resembling the two sounds of the heart.

It is evident that, to determine so delicate a point as the cause of the first sound of the heart, there must not be (as in the experiments of Hope, Williams, and the Committee of the British Association) any rude interference with the mechanism of the heart's action; its cavities must remain untouched; there must be no thrusting fingers into auricle or ventricle (as in theirs); no hooking back of valves (as in theirs); in fact, not one source of sound substituted for another, nor two or more brought together to discover one. My proceedings were as follow.

Large dogs were obtained, and, as in my preceding experiments, the heart was exposed, and the circulation kept up with artificial respiration. A stethoscope being applied to the organ, the sounds were distinctly heard. The superior and inferior venæ cavæ were now compressed with bull-dog forceps, and the pulmonary veins by the finger and thumb; the heart continuing its action, a stethoscope was again applied, and neither first nor second sound was heard. After a short space of time the veins were allowed to pour their contents into both sides of the heart, and both sounds were instantly reproduced. The veins being again compressed, all sound was extinguished, notwithstanding that the heart contracted vigorously. Blood was again let in, and both sounds were restored.

I have thus frequently *interrogated* the same heart for upwards of an hour, and always with the like result.

This experiment (which was, at the time of my first performing it, in 1851, carefully examined, and verified by the late Dr. Marshall Hall and Messrs. Lane and Blenkins; more recently, and during the quiet of the night, by Messrs. Bickersteth, Waters, Fletcher, Grimsdale, M'Cheene, and Webster, of Liverpool; and lastly, for his own especial conviction, by Dr. Quain, of the Brompton Hospital, and again, more recently, by many others of repute) reduces the origin of the sounds either to the vibrations of the valves or to the rush of blood into and from the heart's cavities. That they need not be referable to the blood (that is, not generated in the fluid) is rendered certain by Brakyn's experiment, in which there was nothing but the *tension* of the valves to produce sound; and I will not admit that the flow of blood either into or out of the heart (in health) can produce sound; for, if such were the fact, sound should be developed during the filling of the auricles, and subsequent injection and distension of the ventricles, that is, during the whole pause, which is not the case. But ascribing, as I do, with Billing, Rouanet, Bryan, Valentin, Baumgarten, and others, the sounds to valvular tension alone, it becomes easily explicable why the first sound should be more prolonged than the second; for, although the tension of the auriculo-ventricular valves is instantaneous, the vibrations of their flaps and of the chordæ tendineæ, by reason of their unequal sizes and attachments, are continuous; whereas, from the similarity of size and attachment of the semilunar valves, the sound resulting from their tension is limited.

That the blood closes both sets of valves is beyond dispute; that the force of the ventricular contraction is imparted to the blood, and by it transmitted in all directions, must also be admitted. The valves between the auricles and ventricles being, as we have before seen, already in position, their resistance to the ventricular contraction must necessarily be accompanied by extreme tension and vibration of their flaps and tendons *(first sound)*. At the same time the semilunar valves give way, and the arterial trunks become greatly distended. Then, the ventricular systole ceasing, this distension of the vessels is overcome by the elasticity of their coats, which second power, reacting upon the blood, again impels it, its backward progress, however, being effectually impeded by the semilunar valves (*second sound*). Whereas no sound is heard when, as in the last experiment, the blood is prevented entering the heart; inasmuch as the valves cannot be acted upon, the flaps of the auriculo-ventricular valves lying side by side, and the semilunar being immovably closed by the pressure of the blood upon their upper surfaces.

I contend, therefore, that the fact of both sounds being destroyed and reproduced by the same means is the greatest proof that they depend upon the same cause, which is simply the vibrations of the valves produced by the backward pressure of the blood, first against the auriculo-ventricular (*first sound*), and secondly against the ventriculo·arterial valves (*second sound*).

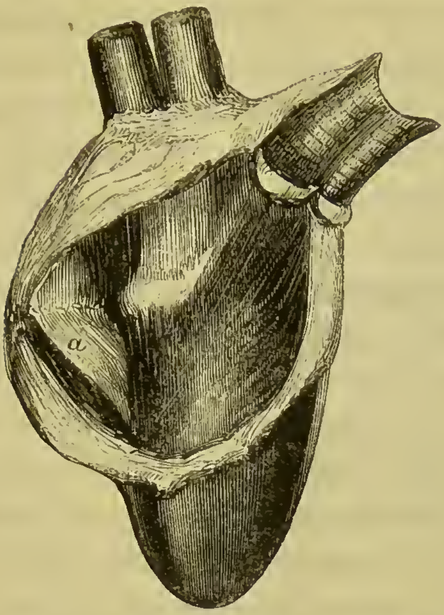
Are there any other means left of confirming my opinions? I crave the reader's attention.

It has been all through stated that valvular tension was the cause of the heart's sounds; that the difference between the first and second sound depended upon the difference in size, shape, and structure of the valves separating the ventricles from the auricles on the one side and from the pulmonary artery and aorta on the other. From this it results that all animals with hearts constructed as our own should give out sounds similar to our own; and upon inquiry we find that it is so, that the sounds heard in the ox, horse, ass, dog, cat, rabbit, &c., differ only in degree, but not at all in character, from the sounds heard in man.

Again it should follow that animals with hearts constructed differently from our own should give out sounds differing from our own; and again upon inquiry we find it is so; for, In birds there is no perceptible difference between their first and second heart sounds. Two sounds are heard, but they are so similar as to appear like one continuously intermitting sound. And why is this? Because the area of vibration for the production of both first and second sound is about equal, *i. e.*, a bird, in comparison with a mammal, has only half an element for the production of the first sound, only one *tendinous* valve separating the auricles from the ventricles (that being between the left auricle and the left ventricle); whilst in the right ventricle the valve corresponding to the tricuspid of mammals is absent, its place being supplied by a triangular layer of *muscular fibres* connected to the margin of the auriculo-ventricular opening, but which, however accurately it may close the opening during the ventricular systole, must in the production of sound be taken at *nil.*

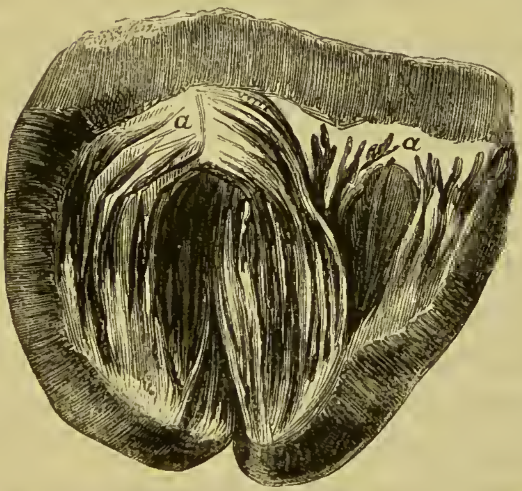
But in birds, as in mammals, the orifices of the aorta and pulmonary artery are both furnished with three semi-lunar valves, consequently there is no difference between the second sound of the heart of an eagle and of a child, of a fowl and of a dog; but the heart of the eagle and of the fowl, wanting, as it does, the *tendinous* tricuspid of the right side, gives out only half the intensity of the first sound elicited from the heart of the child and of the dog. I repeat, therefore, that the sounds of the heart have their origin in valvular tension alone. Q. E. D.

Fig. 14.



Heart of Bird. Right ventricle laid open to show, *a*, the muscular valve.

Fig. 15.



Left ventricle of heart of Bird, laid open to show, *a*, *a*, the tendinous valve.

But to pass, if possible, to further proof.

There is a Family of Birds, the *Brevipennes*, including the Ostrich and the Emeu, characterised by peculiarities of structure approximating its members in some degree to the mammalia. It will be sufficient for my purpose to refer to these peculiarities generally, but to the structure of the heart more particularly. In Knight's "Museum of Animated Nature" the Ostrich is thus spoken of:—"The resemblance of the Ostrich in many structural peculiarities to the ruminating quadrupeds was not overlooked by the ancients, which led them to assign to it the name of camel-bird, in allusion to certain points of analogy between it and the camel: indeed, Aristotle asserts the ostrich to be partly bird and partly quadruped, and Pliny observes that it may be almost classed with beasts. The voluminous thighs divested of feathers are more like those of a quadruped than a bird; added to which the bifid hoof-armed foot, well padded beneath, bears a marked resemblance to that of a camel. In this animal there is a large callous pad on the chest, upon which, when reposing, it throws a great portion of the weight of the body. In the ostrich the sternum, which has no keel, but is simply convex and shield-like, is also covered with a callous pad, or elastic cushion, having a hard rough surface unclothed with feathers, and on which the bird rests while reposing. The eyes, with their long lashes and over-hanging brow, are also camel-like. The vast size and sacculated form of the proventriculus, with its extraordinary apparatus of glands for pouring out a solvent fluid capable of reducing the coarsest vegetable aliment, is not to be overlooked. Nor ought we to pass unnoticed the comparatively developed condition of the diaphragm." Added to which, Cuvier says they have "a vast receptacle in which the urine accumulates, as in a bladder."

We are not surprised, then, to find that the heart participates in this gradual approach to the mammalian type. Instead of there being only one muscular valve separating the right ventricle from the right auricle, there is added a second, partly tendinous and partly muscular,* both of which diverge from a carnea columna springing from the wall of the ventricle. With the exception of this approach to the mammal, the rest of the ventricle is bird-like. Moreover, around the auriculo-ventricular opening there is a more or less distinct zona tendinea. The left ventricle is five or six times thicker than the right; projecting from its walls are three strong muscular masses, from the blunted extremities of which proceed *enormously strong* chordæ tendineæ, which are attached to the apices, under-surface, and circumference of an equally powerful tendinous valve, disposed somewhat like that called the mitral in the human. The aortic orifice is guarded by three strong semilunar valves, and the pulmonary artery by three others proportionally weak. The auricles approach also the mammalian type, the left having a well-developed appendix full of musculi pectenati.

What, then, must be the character of the sounds of the heart of the ostrich?

Look at its enormously strong left auriculo-ventricular valve, resisting the pressure of the equally enormously strong ventricle, which has to send its contents to the greatly elevated head and to the far distant toes. I take not into consideration the more or less sonorous semi-tendinous valve in the right ventricle, when I say that the first sound must differ greatly from the second, and from the same sound heard in dissimilar birds. If I tell the reader that the left ventricle of the emeu (the heart being of about the same size as the human) contains a valve and chordæ tendineæ of twice if not thrice the thickness of those in the human subject, he will be prepared to know that the first sound in the ostrich's heart differs little, if at all, from that of the human, neither does the second sound, but that it is, perhaps, less clear. Of course, I cannot describe sounds; but I have listened repeatedly, and for a long time together, to the heart of the ostrich, and find its sounds approximating to those of the mammal; they may be pretty well imitated by *loob-dub.* The heart beats between seventy and eighty times per minute.

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* This valve is usually described as muscular; but, in fact, it consists of tendinous fibres prolonged downwards from the zona tendinea, coated with muscular fibre. Professor Quekett kindly placed a portion of the valve under the microscope, and confirmed my opinion.

One step further into this inquiry. There is a bird, the Apteryx, *whose respiration is alone thoracic; whose diaphragm is so developed as to want but a few fibres to make it perfect;* none of whose bones are hollow; whose wings are only rudimentary; and whose habits are more mammalian than bird-like. If, then, the respiratory apparatus (with the development of which I have throughout this essay connected the structure of the heart) differs from that of all other birds, its heart must do so too; and, if it be so, the sounds must likewise differ. Let us see.

Professor Owen, in the second volume of "The Zoological Transactions," in describing the anatomy of the apteryx, says: "The principal deviation from the ornithic type of the structure of the heart is presented in the valve at the entry into the right ventricle. This is characterised in birds by its muscularity and its free semi-lunar margin. In the apteryx it is relatively thinner, and in some parts semi-transparent and nearly membranous; a process, moreover, extends from the middle of its free margin, which process is attached by two or three short chordæ tendineæ to the angle between the free and fixed parietes of the ventricle. We perceive in this mode of connection an approach in the present bird to the mammalian type of structure analogous to that which the ornithorhynchus, among mammalia, offers, in the structure of the same part, to the class of birds: for the right auriculo-ventricular valve in the ornithorhynchus is partly fleshy and partly membranous." By "membranous" Professor Owen means what we human anatomists commonly call tendinous, as will be seen from his following remarks: "There was nothing worthy of note in the left ventricle, or in the valves interposed between it and the left auricle; the two membranous flaps presented the usual inequality of size characteristic of the mitral valve in birds."

Through the kindness of Professor Quekett, of the Royal College of Surgeons, I have been put in the way of seeing the hearts of nearly all animals; among the rest, that of a small specimen of the apteryx; and, in addition to what has been quoted from Professor Owen's paper, I will add, that from the wall of the right ventricle proceed two distinct carnæ columnæ, from which the chordæ tendineæ pass to the tendinous flaps of the valve. The left ventricle contains carnæ columnæ of the mammalian type and disposition. The left auricular appendage is nearly, if not quite, as large as that of a mammal. In fact, here is a bird with a heart nearly identical in structure with that of a child or of a dog. Then its sounds must be like those of the child and of the dog, and differing greatly from those of the eagle. It is even so, as I have ascertained. If any one will listen to the heart of the apteryx, he will almost think it is the heart of his own little child, or of his favourite dog. Its pulse, I should think, is not above 100 when at rest; as with me under a little excitement it varied from 100 to 120.

Above all, let the reader listen first to the heart of the eagle, then to that of the apteryx, and he will find the differences I have attempted to describe, and at once reconcile them with the teachings I am inculcating, viz. of their valvular origin alone.

But to anticipate a little. (*See below.*) If either "*Bruit musculaire*," "Impulse of the heart's apex against the ribs," "Rush of blood into and out of the heart's cavities," "Collision of blood against the orifices of the aorta and pulmonary artery," or "The concussion between the active and passive portions of the blood," were the cause of the first sound of the heart, why do the sounds produced by the action of the eagle's heart differ from those produced by the action of the child's heart? Blood is passing into and out of either heart in exactly the same manner; auricles and ventricles are contracting with the same rhythm and comparative force. Why then any difference between the first sound of the heart in the one and in the first sound of the heart in the other? Why, simply because the bird has one vibrating auriculo-ventricular valve at the instant of the first sound, whereas the mammal has two. (9).

It would be impossible, within the limits of this essay, to criticise all the views that have been put forth in explanation of the cause or causes of the heart's sounds. Some of the opinions are well worthy of consideration, and can only be put aside by observation and experiments tending to their contradiction,—others are little else than fancies, directly opposed to known physical laws. The assigned causes, I believe, worthy of investigation are:

1st. *Bruit musculaire;* 2nd. Impulse of heart's apex againt the ribs; 3rd. Rush of blood into and out of the heart's cavities.

1st. With respect to *bruit musculaire*,—If it and the first sound were identical phenomena resulting from the same cause, viz. muscular contraction, it should follow that in cases of hypertrophy of the heart, in which the cause is increased, the effect (the first sound) would be increased also; but the reverse is the fact; therefore, they are not identical phenomena.

Again, that they are not identical is proved to demonstration by my last experiment, in which, although muscular action is going on vigorously, no sound whatever is heard. The explanation of the diminished sounds in hypertrophy is that the sounds, being valvular, are less audible through thicker than through thinner ventricles.

Indeed, much, if not all, of what is usually put down as *bruit musculaire* is nothing more, in my opinion, than the vibrations of the compressed air in the tube of the stethoscope, or in the external auditory meatus, produced by the impulsion of the instrument or some other solid against the ear, or by friction between the two; for a low rumbling sound can be produced by pressing a handkerchief or the corner of the pillow against the ear, or by slightly pressing the tragus into the meatus, and another sharper sound is developed by its sudden disengagement on the removal of the finger. On this subject the late Dr. Bellingham has a very good remark; speaking of the so-called *bruit musculaire* heard at night during the contraction of the masseter and temporal muscles, he says that it is only heard by the ear resting on the pillow, and not at all by the other; a fact I have frequently verified, and which is explicable only upon the supposition that the sound results from the contact of the ear with the pillow. Now, all such adventitious sounds may be developed at the time of the ventricular contraction, or beat of the heart, and the more awkward the auscultator, the more are they likely to arise; but that the first sound of the heart is due to muscular contraction, and identical with *bruit musculaire*, is, I think, logically, experimentally, and anatomically disproved.

2ndly. The striking of the heart's apex against the ribs has been given as the cause of the first sound. But, since the apex is not tilted forward, and inasmuch as the blow is only apparent, and is the result of the noiseless change of form of the ventricles within the pericardium; and, further, since the sounds are more audible after the walls of the chest have been removed than before, this at first sight probable cause becomes in fact no cause at all.

3rdly. The passage of the blood into and out of the heart has been assigned as the cause of one or both sounds of the heart.

Several modifications of this cause have been entertained by different authors, as, for instance, "shock of blood against the ventricular walls;" "collision of blood against the orifice of the aorta and pulmonary artery;" "concussion between the active and passive portions of blood;" "rush of blood," &c. (10).

One author's opinions only I shall examine at some length, for the value of one represents pretty nearly the value of the whole.

The theory of the late Dr. O. B. Bellingham, of Dublin, it will suit my purpose best to criticise; first, because his work is of recent publication, and has, I believe, the confidence of a great portion of the Profession; and, secondly, because the views he there sets forth are diametrically opposed to the valvular theory of the origin of the heart's sounds, which it is my present purpose to establish. He says,—

> The first sound of the heart we know is synchronous with the ventricular systole; in this act the blood, compressed by the contraction of the powerful muscular walls of the ventricles, is propelled with considerable force into the aorta and pulmonary artery, the sigmoid and semilunar valves of which are suddenly

elevated. In the rapid passage of blood from a wider to a narrower area, there must be considerable friction between this fluid and the parietes of the arterial orifices, quite sufficient, in my mind, to produce the prolonged first sound of the heart. This sound has necessarily a distinct character from the second sound of the heart, because the resistance to be overcome is so much greater, and the passage of blood through these orifices is more gradual; it is likewise more prolonged, because sound must be developed during the entire period that the blood is passing from the ventricles into the large arteries; and the slower the action of the heart the more prolonged will the sounds be.

To this I object, that it asserts Nature to be at fault, which is impossible. There is no such disproportion (in health) between the orifices of the vessels and the cavities from which they spring; they are gradually continuous one into the other; the aorta and pulmonary artery, by virtue of their perfect elasticity, increasing in area as the capacity of the ventricles is diminished, as shown in Fig. 10, page 14. There is not "considerable friction between the blood and the parietes of the arterial orifices," so long as vessels, valves, and ventricles remain healthy. When either the mobility of the valves or the elasticity of the arterial coats becomes impaired, then, I grant, sound may be, and constantly is, produced. But what can exceed in smoothness the lining membrane of the heart and arteries? There cannot, then, be friction, which would presuppose an error in the mechanism—an exception in the Divine Author's works. Again, the heart has sufficient work to propel the blood to the distant capillaries without meeting with obstruction at the instant of its effort. Lastly, where is the "friction between the fluid and the parietes of the arterial orifices" in Brakyn's experiment? and yet the first sound is heard, resulting from valvular tension alone.

Of the second sound he thus speaks:—

> The second sound of the heart we know is synchronous with the ventricular diastole. During this act the muscular fibres of the ventricles are relaxed, the cavity of the ventricle enlarges, and the walls of the ventricles re-expand; the curtains of the auriculo-ventricular valves open, and there is a sudden influx of blood from the auricles through the auriculo-ventricular orifices. It is scarcely necessary to say that it is not the contraction of the auricles which propels the blood into the ventricles at this period of the heart's action; nor is the dilatation of the cavities of the ventricles the result of the entrance of blood from the auricles, as some have supposed. It is not, either, necessary for the production of this sound that the diastole of the ventricles should be an active process like the systole; the ventricles being hollow muscles, the state of relaxation of their muscular fibres is a state of dilatation of their cavities; hence a vacuum would be created in them if the auricles were not at this moment full of blood, ready to supply them; but, as the latter had been filling during the whole period of the ventricular systole, this cannot happen, and the blood passes through the auriculo-ventricular orifices in a full and rapid stream, and with sufficient force to generate sound.

According to Bellingham, then, the second sound is produced by the rapid flow of blood from the auricles into the ventricles, and this blood is drawn into the ventricles by the force commonly called suction, owing to the state of relaxation of their fibres, which, he says, "the ventricles being hollow muscles, is a state of dilatation of their cavities."

In the first place, I object to the term "hollow muscles," as likely to mislead and perplex; for the ventricles are never hollow during life, their inner surfaces being either in contact with each other or with the contained blood. Then as to the state of relaxation of their fibres being the state of dilatation of their cavities, see how this is completely disproved by my last experiment, where, the blood being shut out of the heart's cavities, the heart continues, nevertheless, to act vigorously. Here no dilatation can take place, for how—when the flow of blood through the heart is stopped, and hence all force from within removed—can the ventricular walls expand, how can the ventricular cavities be dilated? It is a physical impossibility (tending to a vacuum); and yet the globular form of contraction and the lengthened form of relaxation are distinctly seen, but no dilatation can take place. The phenomena are as follow: So soon as the ventricles have ceased their contraction, relaxation of their fibres commences. This occurs previous to the dilatation of their cavities, and must not be confounded with it: for relaxation of the muscular fibres of the ventricles has no more to do with dilatation of their cavities, or with the passage of blood into them, than relaxation of the fibres of the sphincter ani has to do with the expulsion of fæces from the bowel; yet in both cases the same passive yielding state of the relaxed muscular fibre is necessary to complete the act. As the abdominal muscles, through the medium of the fæces, dilate the sphincters, so do the auricles, through the medium of the blood, dilate the ventricles.

Again, the blood does not, as Bellingham says, pass at the time of the second sound in a full and rapid stream into the ventricles. If it were so, it would follow, that, by the time the auricles next contracted, the ventricles, as well as they, would be full; in fact, there would be full auricles and full ventricles by the end of the diastole; and the next contraction of the auricles (which he admits, with every one else, immediately precedes that of the ventricles) would tend to arrest the systemic and pulmonary circulations, forcing the blood back into the venæ cavæ and lungs, for it is certain their force could not be exerted on the already distended ventricles; and it is also equally certain that the auriculo-ventricular valves could not act otherwise than to force back the blood flowing into the auricles upon the venous and pulmonary systems, seeing, as has before been shown (pages 15–18), these structures form a perfect septum between the auricles and ventricles as soon as the latter become distended. In fact, such a heart as Bellingham describes we meet with when the ventriculo-arterial valves are so diseased as to permit of regurgitation; in which cases, the ventricles being imperfectly emptied, the auricles, which have their supply ready to inject into the ventricles, have part of the blood thrown back upon them, and thence into the venæ cavæ and pulmonary veins, producing those distressing symptoms we are too well aquainted with.

Bellingham is not the only one who questions the function of the auricle being to distend the ventricle. There have been from time to time various modifications of this dilating or self-filling power of the ventricle put forth. It will be worth while, then, to devote a few lines to consider attentively what may be the use of the auricle. (11).

There is no one existing, I suppose, who would deny that the blood returns from all parts of the body by the venæ cavæ to the right auricle, and from the lungs by the pulmonary veins to the left auricle.

No one either can deny that in some animals this return of the blood is effected in a shorter time and by a more rapid current than in others.

It should follow, then, if the use of the auricle be to distend the ventricle, that in animals of slow respiration and circulation the auricles would be large, in order that their muscular power might urge on the sluggish stream into the ventricles. By the same rule it should be, that, in animals with quicker respiration and circulation, the auricles should be smaller; and lastly, that in animals with the most highly developed respiratory and circulatory systems the auricles should be smallest, requiring to give only a little impetus to the already impetuous fluid.

To sum up, I would state that—

The size and strength of the auricles should be in inverse proportion to the rapidity of the circulation and respiration, and to the consequent muscular energy ;

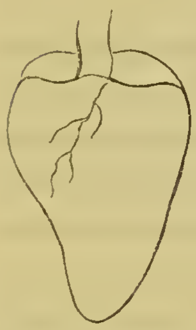
and, as far as I am aware, it is so, for—

In the bird, they are small ;

In the mammal, they are larger ; and

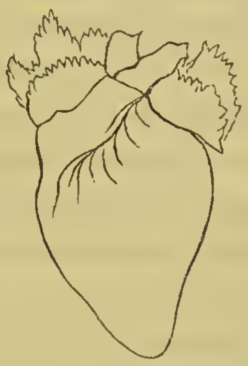
In the reptile, they are largest, being relatively of double the strength and size of the auricles in either other class.

Fig. 16.



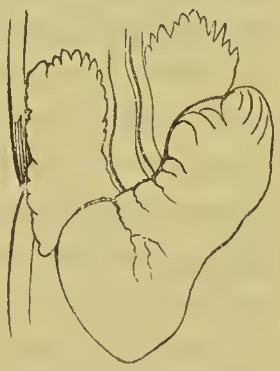
Heart of Domestic Fowl.

Fig. 17.



Heart of Hare.

Fig. 18.



Heart of Snake.

Of what use, then, can the auricles be, unless to distend the ventricles ?

Let us now hear Bellingham's objections to the valvular origin of the first sound. He says: "Now the first sound of the heart cannot have its cause exclusively in the sudden tension of the auriculo-ventricular valves, because this act takes place at the very commencement of the systole; and the first sound is a dull, prolonged sound, which persists during the entire systole."

As this same objection was made by the Committee of the British Association appointed to discover the origin of the sounds of the heart, I cannot do better than answer it in the words of Bryan, when combating the conclusions arrived at by that learned body twenty years ago. On what grounds, he asks, is it stated that the cause of the first sound is of equal duration with the sound itself? The sound of a drum is of longer duration than the blow which caused it; a harp-string vibrates long after the finger has struck it; and, though the shock of the sudden tension of the ventricular valves be instantaneous, yet the vibrations excited by the shock of sudden tension may continue." To this I shall only add, that the unequal lengths and sizes both of the flaps of the valves and of the chordæ tendineæ fully account for the prolongation of the sound.

I have thus shown that Bellingham's theory of the heart's sounds is incompatible with the perfect action of the organ, and also that his objection to the theory of the valvular origin of the heart's sounds is founded upon a total mis-apprehension of the subject, and consequently is of no worth whatever.

There is yet another road open by which inquirers may confirm the truth, viz. post-mortem examination. I would suggest that careful notes be taken of cases of morbus cordis, and the sounds heard during life put side by side with the condition of the valves after death. The heart, however, should not be examined as is usually done; it should be tested hydraulically, as in fact it is during life; and then I feel certain we should soon arrive at a comprehension of all morbid sounds (*bruits*), and at a just estimation of their importance. Any London curator giving his attention to this subject would assist the Profession materially. The following method I would recommend:—

Let the heart be removed with an inch or two of the large vessels, then cut away the auricles almost close down to the auriculo-ventricular openings, carefully take away the clots from the ventricles without injuring the delicate chordæ tendineæ, for if even one of them be injured the perfect adaptation of the flaps of the valve is prevented.

First shut down the semilunar valves with a stream of water, (the coronary arteries at their origin must be tied,) then pour water into the ventricles, and the valves, if healthy, will rise upon the surface of the fluid in proportion as the ventricles become filled, and form a perfect septum between them and the auricles. Should the semilunar valves be so diseased as not to permit of closure by fluid, the aorta or pulmonary artery, as the case may be, should be tied, or a cork inserted into it, before the mitral or tricuspid valve is tested.

The above is the mode adopted by myself; others can of course use their own methods.

In conclusion, I would urge upon the student of Medicine the necessity of thoroughly mastering the mechanism of the heart's action, the time and order of the occurrences constituting the rhythm. Only by so doing will he comprehend the morbid conditions he will be called upon to combat, and the sufferings he will be expected to alleviate. To this end the Table at p. 19 has been drawn up, which I hope will prove useful. As a starting point in the study of both functional and organic diseases of the heart, let the student adopt the following simple exposition of the healthy sounds, and all mordid sounds will quickly become intelligible to him.

When listening to the heart, two sounds are heard, the first long, the second short; they may be tolerably well imitated by the sounds *lub*, *dup*. When the first sound is heard the ventricles contract, and the auriculo-ventricular valves are made *tense*, and completely separate the cavities of the ventricles from the auricles; the *tension* of these valves produces the first sound. When the second sound is heard the ventricular systole has ceased; the aorta and pulmonary artery have reacted upon their contents; the cavities of the ventricles are separated from the systemic and pulmonary systems by the closure of the semilunar valves, the *tension* of which produces this sound. This is the true mechanism of the valves, and a sufficiently physical explanation of the origin of both sounds, none other, in my opinion, being reconcilable with the teachings of Natural Science.

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NOTES.

(1). This systolic impulse has been aptly likened by Billing to the sensation felt when applying the hand to the (masseteric portion of the) cheek whilst the jaws are firmly closed, and the muscles alternately relaxed and put in action, as in mastication.

(2). When the auricular attachment of the ventricle descends, moves forward, and approaches the apex, the left auricle steps forward and downward into the place just occupied by the ventricular base.—SIBSON, *loc. cit.*

(3). Speaking of the movements of the heart in the ass, Sibson says that during the ventricular systole the aorta and pulmonary artery "are drawn down about a quarter of an inch by the neighbouring descending fibres."

(4). This experiment was several times tried by Mr. Waters of Liverpool and myself, in order to determine, if possible, whether there was, as stated by Sibson, any "zone of comparative rest" in the ventricular walls; but we could find none. I have since frequently repeated and modified the same experiment, and always with the result stated in the text.

(5). Valentin's Physiology, translated by Dr. Brinton.

(6). Monthly Retrospect of Medical Science (Jan. 1849).

(7). Hunter's quick mind anticipated the remarks in the text, for, speaking of the auriculo-ventricular valves, he says, "They are more complex, requiring an additional apparatus to make them answer the intended purpose; for it is necessary that their loose floating edges should be restrained from inverting themselves into the auricle upon the contraction of the ventricles. This is done by tendons, which are fixed at one extremity along the edge of the valves, and at the other to some part upon the inside of the ventricle. The tendons which are longest are inserted into columns of muscle, the intention of which is very evident: for, if they had gone the whole length in form of a tendon, they would have been too long when the heart contracted, and the valves in such a case would have allowed of being pushed into the auricles, so far as to admit of the blood escaping back into the cavity; but the carneæ columnæ keep the valves within the ventricle, in the contracted state of the ventricle."

(8). This we have seen is scarcely correct; the valves are closed before the ventricular systole, which, however, as Billing says, causes them to become tense and produce the first sound. With this slight exception all he says on the subject is perfectly true.

(9). My best thanks are due to Mr. Bartlett of the Zoological Gardens for assistance in listening to the hearts of large reptiles, birds, &c. Mr. Bartlett will corroborate me as to the character of the sounds of the eagle's heart.

It must not be supposed that I allude to the sounds of the heart in a frightened or excited bird ; no such thing: Mr. Bartlett selected a powerful Australian eagle (*aquila audax*) that had become perfectly tame and friendly with those to whom he was accustomed, his keeper holding him in his arms, and permitting me to auscultate without fear. Mr. Bartlett explained to me that the bird was naturally too bold to be frightened, and too confident in his keeper to be excited; consequently the heart was acting normally. I listened also at the Gardens for the sounds of the heart in the Python Sabæ and Guinea Rock-snake, as well as to the Alligator (*alligator lucius*) and the Snapping and Edible Turtle, but in no instance could I clearly detect any sound. I shall, however, return to the inquiry.

I may mention, that, before I had listened to the heart of a bird, I predicted to Mr. Bartlett and to a friend what would be the character of its sounds, solely from an examination of its anatomical structure, and the result verified the accuracy of my opinion.

(10). Harvey considered that the passage of the blood produced sound at the time of the ventricular systole ; he says, " Ita dum istis cordis motibus fit portionis sanguinis et venis in arterias traductio, pulsum fieri et exaudiri in pectore contingit."—*De*

Motu Cordis, cap. quintum. He nowhere else alludes to sound, and does not appear to have recognised the second sound.

(11). Harvey, in describing the heart's action, gives the auricle its proper share of the phenomena: he says, "Primum sese contrahit auricula, et in illa contractione, sanguinem contentum in ventriculum cordis conjicit."

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